Department of Genetics
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Dr. William Hayes
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Dear Dr. Hayes:

Thank you for your note of December 3. I am especially pleased to hote your interest in recombination problems.

I think I can confirm your observations with respect to the action of streptomycin on sexual fertility. Not long aga, we did some crosses of 58-161 x W-1177 (= W-677 S^r) in which the 58-161 parent was exposed to streptomycin, so as to substantially reduce, but not entirely remove its viability. Yields of prototrophs were not greatly affected, and, indeed, considerable yields have been obtained in crosses conducted on minimal + streptomycin agar. We did not, however, have occasion to conduct the reverse cross, and perhaps for that reason have not drawn any definite conclusions from the experiment. I would still suggest that the effect (or lack of it) of sm could be expressed in simpler terms: that the treated cells are incapable of sustained proliferation in clonal reproduction, but can still congress with other cells with respect to sexual function. The greater sensitivity of W-677 is just that. One reason that I have not sustained the suggestion of a unilateral exchange is, for example, that K-12 can readily be crossed with B-M-S^r or T-L-B_1-S^r (by plating on minimal-sm agar, with or without prior growth in mixed culture), and the latter can also be crossed by the usual method. I do not quite see the bearing, one way or the other, of your result on the possibility that "the agent of genetic recombination is exuded" in any way, although this is, of course, as good a working hypothesis as any for further work.

I regret that I cannot accomodate your request for K-12 auxotrophs that do not overlap the BM or TLB_1 series. We have had some rather unsatisfactory cultures that would not be very useful, and just now are engaged in reisolating a new series, with special attention to avoiding the use of mutagenic agents. But this is part of a program to improve our stocks for crossing some thirty distinct, interfertile isolates of E. coli from various sources, and it may be some time before they are ready. I must rather shamefacedly confess that virtually all of our work with K-12 has involved descendants of Tatum's first two mutants (58 and 679 of his 1945 paper), and this is not the first incident to make us wish we had made our mutants extensively, as well as concentrating them in lines like W-1177. If I can guess your intentions, however, you might be able to use the ancestors of W-677, 679-680 and Y-10 (T-L-; T-L-B_1- respectively). I have not looked at these closely for some while, but trust that the cultures we shall send you will be satisfactory.

The culture "S", described by Weigle and Delbruck may be any one of several that we sent them; their records have apparently been lost. Mrs. Lederberg will, however, include W-1485. This is a lambda-sensitive "mutant" isolated from K-12 after UV. You may be interested that NTCC-123 (Cavalli's interfertile culture) is also sensitive to lambda.

I was pleased to note that you had cleared up the strain-component of the UV effect. My experiments to confirm the UofTexas report gave very sporadic, though over-all incontestable, results. The question of a true evaluation of rates of recombination is very tricky, and an easily reproduced experiment is essential.